MR. A. FRANKLIN SHULL, assistant in zoology in Columbia University, has been appointed acting assistant professor of zoology at the University of Michigan, to succeed Dr. A. S. Pearse, who has gone to the University of Manila.

MR. DUNHAM JACKSON, now studying at Göttingen on a Harvard traveling fellowship, has been appointed instructor in mathematics at Harvard University.

AT the Normal College, New York City, Charles T. Kirk, instructor, has been promoted to be assistant professor of geology, and Miss Emily O. Long, to be assistant professor of botany.

DISCUSSION AND CORRESPONDENCE

THE METHOD OF SCIENCE, A REPLY

A RECENT number of SCIENCE (January 27, 1911), has a forceful address by Dr. Minot on the "Method of Science." It is a new presentation of a topic fully discussed from the attitude of pre-evolutionary thought, but in such a manner and from such premises that its logic can not serve as a basis of present problems. I do not have the feeling of disrespect for the old thought that Dr. Minot seemingly has, but I agree with him that its principles and methods give little help in solving the problems science now faces. But at this point our differences begin, for in his restatement of principles, admirable as it is, he cuts the ground from under the social sciences by putting up standards that they can not meet. I do not think he meant to do this, yet the feeling he shows against the old philosophy warrants the inference that he would pass a similar judgment on the results of social science.

A new statement of the laws of thought is certainly needed. Early logic was devised by the theologians to prove the existence of supersensual units. As instruments for this end the so-called laws of thought are effective. But we need other rules to solve present problems. Not only is this so, but the methods of investigation have been so altered during the past fifty years as to create new problems. Accurate measurements are a new device. There were cases of accurate measurement before the present epoch, but they were not numerous enough to create a peculiar type of reasoning and thus to force a revision of the rules of logic.

The old division was between inductive and deductive logic. This controversy is now practically dead and in its place is arising one between inductions based on observation and those on experiment. Observations are generalizations under complex conditions, while experiment means isolation, simplicity of environment and accurate measurement. Workers in physical science distrust observations and demand in their place carefully verified results. This change is not a matter of theory, but due to practical situations faced by scientists in their various fields. The new medicine of which Dr. Minot is so good a representative gives an excellent illustration of the situation that forces him to attempt a reformulation of the laws of thought. The old practitioner was an observer: he diagnosed cases from symptoms. The new school experiments and measures. To say that science is exact measurement means practically to shut out the old physician who carried his knowledge in his head and whose office was not a chemical laboratory.

But if the laws of thought needed to shut diagnosis out of medicine are formulated as general laws, rules are set up that exclude all social judgments derived from observation. The tendency to do this is already visible in biologic sociology whose premises are taken bodily from biology. Bold deductions are made and conclusions drawn that sweep aside all generalizations based on observation. Here is a sample of reasoning of which we will have many more if Dr. Minot's rules of thinking win acceptance. I quote from a recent magazine article. "No generalization has ever exercised such a far-reaching effect on thought as the theory of natural selection. It is hardly necessary to point out that the corresponding belief in sociology is that all progress must come from the gifted individual, from the 'sport' who survives as the best of his kind. Darwinism lays stress not on the democratic

mass and their comfort, but on the few men of talent and their incomparable value to society." The essence of this position is the same as Dr. Minot's[1] exact well-proven premises, a distrust of observation and the conviction that scientists seldom err except in their measurements. This leads to long-range deductions and a neglect of verification within the field, where the conclusions are drawn.

Such methods reflecting the growing tendency in science to disregard observation force those who use it to rise in its defence. A controversy of this kind could not have arisen earlier because no one then questioned the validity of observation. If, however, science has come to mean exact measurement and laws of thought are formulated in harmony with the new view, observation must also have its laws restated or its results will be questioned not in a few fields but in every part of scientific research.

When we seek to demark the field of observation from that of experiment, it will be seen that experiment is carried on under local specific conditions. Only when an object is isolated and its environing conditions definitely fixed can the accurate measurements be made upon which the success of an experiment depends. Observations, however, are made under complex conditions and usually they cover a large area of space or time. The essence of an observation is to fix on some mark or characteristic of an object through which it may be recognized. Reasoning through observation joins two such marks in the relation of cause and effect. If "X" is always followed by "Y" the object of which "X" is a part is the cause of the object or event of which "Y" is a mark. In observation wholes are thought of in terms of some of their definite marks and thus reasoning becomes a connecting of these marks in some casual relation. In contrast to this procedure the "X" and the "Y" of an experiment are isolated from the wholes to which they ordinarily belong. Their qualities and relations can thus be accurately measured and described.

If this is the difference between observation and experiment, the mode of thinking used by the workers in the various sciences can also be contrasted. There are four types of reasoning whose peculiarities depend upon the use made of observation and experiment. The first group affirms that science is measurement and thus rules out observation. This group of thinkers is of recent origin because the means of accurate measurement are a recent discovery. It is no wonder that Dr. Minot found books on logic useless, for early logic gave rules for observation and deduction but did not recognize measurement as a means of investigation. Now we have whole sciences within which measurement is the main source of progress whose workers are so effectively organized that observers of the older type are frowned down or excluded. A social caste is thus formed who set up standards of their own and who issue a "Who is Who" of learned men from which they exclude those using observation as a method of research.

The second method has become popular through the discussions of heredity that Dr. Weismann began. This assumes that if we have two series of events both measurable and certain the first is not only the cause but the sole cause of the second. If, for example, there is a definite alteration of the hand it must have been caused by an antecedent modification of the germ cell of the organism. All observations of the hand and all other sources of modification except those of germ-cells are shut out and a bold deduction is put in their place. The method of this group thus includes nothing but measurement and bold unverifiable deductions. They are playing havoc in the social sciences because their deductions become the basis of biologic sociology.

If we pass from the sciences using experiment and measurement to the social sciences that depend on observation, we likewise find

[1] I refer to such sentences as these: "A broad examination of the method of science reduces itself to the study of the general principles of securing accuracy." "It must be doubted very seriously whether the study of logic is really essential for the right training of an investigator." "It is my belief that the logical work of scientific men is usually well done and is the part of their work which is the least faulty."

two groups. The method of investigation correspondingly changes since social science can not readily get at causes by experiment but must begin with observation of results and work back towards causes by indirect methods. This method has lately been renamed pragmatism and involves a judgment of causes through their effects. Consequences are open to observation; causes are not. A cause must therefore be judged by its observed effects. The leading exponent of this method was the late Professor James. I do not wish to defend his arguments, but to call attention to his method. His observations—those on which truth depends—are psychic phenomena. We may therefore call this method psychic pragmatism, for the satisfaction that the perception of truth affords becomes its test. There is, however, another method that goes out from consequences just as Professor James does but which uses objective social tests instead of psychic tests. Social pragmatism uses marks to visualize wholes, but the marks are the objective social consequences of acts which can be measured and verified. Social consequences can be measured and through their observation a steady advance is possible in ways that will put social observation on a par with physical measurements as a means of developing science.

If it can be agreed upon that observation and experiment furnish the only basis upon which investigations can rest the next subject of importance is the canons of reasoning. Dr. Minot assumes that the reasoning of scientists is seldom defective and that their main errors are those of measurement. Scientists however, are as liable to errors of logic as other people and scientific method can not disregard the laws of thought. They are the very essence of good thinking and must therefore be formulated.

The first and primary rule is that only observations and experiments can be used as premises in deductions. This seems an innocent rule, but it involves more than at first sight is apparent. All experiments are local and specific in their conditions. There is no such thing as a general experiment and hence premises derived from them are local definite facts. The same is even more true of observations. They are made by individuals and no one can extend his observations over more than a local field. All valid data are therefore local and specific. Reasoning consists in extending the scope of these premises to other and broader fields. It follows from this that there is no difference in kind between an induction and a deduction. This distinction is due to the well-deserved disrepute into which the dogmatic assertions of theologians and metaphysicians fell. Scientists wanted therefore to get a peculiar mode of reasoning that would avoid these evils. In this they have failed. So many sciences have become deductive that the reasoning of scientists differ in no essential respect from that of any other group. The differences are in the premises and in the verifications, not in the reasoning. There is but one method of reasoning. Its rules apply to all thought and to all subjects. The same end that the distinction between inductive and deductive thought is attained by the second rule of good thinking. No generalization should be used as a premise in reasoning. A generalization is a result of previous thought and is only an approximate truth. Every new chain of reasoning must go back to the original data in the form of observations and experiments and be based on them together with the new data obtained since the original generalization was made. The chief violations of this rule are in social science, but scientists are not free from this error. They, like other people, form themselves into social groups and thus acquire dogmas and prejudices that induce them to use the generalizations of their group as premises when they should confine themselves to their data in the form of observations and experiments. Sound reasoning always goes from the local to the general. Universals are made either by some social group imputing value to a premise that serves their practical needs or they are loose generalizations based on imperfect data. Whatever their source, they are unsafe premises and lead to widespread popular errors.

From this it follows as a third canon of thought that all conclusions need an independent verification. If reasoning from generalizations were permissible and thought could legitimately move from an acknowledged universal to a particular, verifications would not be necessary. It is interesting to see the many ways in which thinkers try to avoid the need of verification. From this temptation scientists are no freer than other thinkers and they have furnished many notable examples of such errors. Verifications are, however, always necessary and they must be based on fresh data. Reasoning merely points out where these data exist and what data are pertinent. The work of getting at the truth is only half done when the conclusions drawn from premises are shown to be valid.

Thought is the connection between two objects or ideas brought about by the similarity or dissimilarity of their inherent elements. Progress in thought consists in passing from indefinite marks of this identity or difference to those capable of definite description and measurement. This fourth rule of good thinking brings out the relation between observation and measurement. Verifications are improved when observations are verified by experiment and experiment by observation. Only in this way can we be sure that the data of the verification are independent of those of the premises. There is a still further improvement when enunciated principles are based on observation and their verification consists of data arising from experiment and measurement. Such proofs are the most stable science can offer. It is a goal that can not always be reached, but it should always be striven for. Principles are most readily obtained from observation; their proof, however, is complete only when measurable data afford them a verification.

A final rule of thought is that no law is to be regarded general unless it is capable of independent statement and verification in many fields of investigation. This is the doctrine of multiple verification. It is often assumed that the way to prove a law is to get more data in some one field. Such a proof is less satisfactory than independent verifications coming from data derived from other sciences. It is the extension and restatements of a law in other fields and by independent investigators that raise its validity above local generalizations which have both time and space limitations. All observations and experiments are of this local character. They need multiple verifications to make them worthy of general acceptance. It is a corollary of this that thought is improved not by additional erudition in a given field, but by the movement of thinkers from field to field. There is a strong tendency to resent such a movement and the intruder is likely to receive rough treatment by his new colleagues. Yet this has been the way in which the greatest victories of thought are won. If Pasteur had not passed from chemistry to medicine his work might have been scientific, but it would not have been effective. Narrow specialization tends to such complete isolation of a group that its activity becomes socially valueless. The mobility of thinkers is the only safeguard against these evils. It makes trouble but it brings results.

Two conclusions follow from the preceding discussion. The first is that there is but one true method of reasoning in the use of which all are equally liable to error. The second is that there are two kinds of data, observations and experiments, both of which must form a part of any complete verification. These general statements would be of little use if they were not applied to the problems that separate social from physical science. While the opposition is general it is focused upon the controversies about man and his relations to the animal world. The one group use as their data the experimental knowledge of animals and then sweepingly apply this knowledge to man. "What is true of dogs is true of men" is a dictum coming from deductive medicine which illustrates the methods of all biologic sociologists. On the other side, there are social laws established by observation which have been accepted by the mass of mankind as rules of conduct: Which of these is right both in method and fact?

In the first place, it should be recognized

that men can not be put in laboratories and experimented on. It would seem, therefore, that observation must have a place in studies of man that it need not have in animal investigations. In the second place, the tests between the two methods do not lie in problems of normal development, but of pathology and degeneration. There are few observations about normality that are worth much. Social observations are mainly about defects and abnormalities. Keeping these facts in mind, the issue is clear. I will state it in the words of Dr. F. A. Woods. "Experimentally and statistically there is not a grain of proof that ordinarily environment can alter the salient mental and moral traits in any measurable degree from what they were predetermined to be through innate influences."[2] To test such a statement it must first be asked what are "mental and moral traits." If Dr. Woods means traits like sympathy, I agree with him. I know of no observational evidence showing it can be altered except by organic development. This may be true of all positive characters. But many so-called characters are not positive traits, but merely conditions. We can not make good men better merely by an environmental change but we can in this way eliminate vice. Is there then a difference between a condition that leads to degeneration and a biologic trait that is necessary for progress? To be specific, are drunkenness, hysteria and criminal tendencies conditions having objective causes or are they biologic characters? Social observers point out what the conditions are that bring on these results and contend that the so-called traits appear and disappear with the presence or absence of given objective conditions. The deductive biologists start with premises about germ cells and apply their conclusions to man without verification. The difference is not one of fact, but of the sufficiency of bold reasoning.

If, as Dr. Minot asserts, scientists seldom err in conclusions when their measurements are exact the weight of authority is with theorists. I am not so sure of this as he is. The weakness seems to me to lie in the difference between the conditions of man's survival and those of lower animals. Before the rise of social sentiments elimination acted sharply against the defective individual, and hence degeneration could not become prominent. Then all traits were traits of survival and few pathological states appeared. Man, however, through sympathy preserves the weak and hence lowers the average man below his normal condition. If we regard one hundred points as the normal level in the animal world the lack of ten points would lead to elimination. In human society, however, a man could lack forty points and yet perpetuate his kind. I do not wish to attempt a mathematical demonstration, but it is plain that human sympathy reduces materially the sharpness of elimination. Sympathy could not act in this way if society did not have a surplus that it used to maintain the defectives. Sympathy is thus the indirect cause of the failure of elimination, but a condition of surplus is its direct cause. If it were absent, only normal people and normal traits would survive.

[2] *The Popular Science Monthly*, April, 1910.

There are two objective conditions that reflect themselves in abnormal traits, a condition of surplus and a condition of deficit. The traits due to a surplus are usually called vices, while those of deficit are called crimes. These terms are not sharply contrasted, but their use is definite enough to illustrate my meaning. Give men more than they need and they sink into vice: take from them what they need and they become criminal. If vices and crimes can be changed or removed by altering income conditions we have proof that vicious and criminal traits are not biologic but economic in origin. We can then conclude that abnormal traits are not true biologic characters, but the impressment of economic conditions which are modified as the environment gives a surplus or deficit to those within it. A condition of deficit desocializes those who suffer from it and thus brings out atavistic traits not appearing in normal persons. A condition of surplus making people emotional, morbid and hysterical undermines the power of the will. Deficit people can be said to be

too willful, while surplus people are almost will-less.

I do not use this argument to show that my position is correct but to make clear what it is on which the contrasted arguments rest. The biologic sociologists are using bold deductive arguments without a verification. Their position has plausibility only by ignoring observational evidence. Deductive medicine with its neglect of diagnosis puts itself in the same position. The one group affirms that what is true of germ cells is true at maturity while the other says what is true of dogs holds for men. This is reasoning, not observation or experiment.

It is said of Agassiz that he took his students out to a great boulder near Cambridge and asked them what they saw on it. Some saw nothing: others saw vague scratches. Only he saw the ice-markings and proof that the boulder was deposited by a glacier. By the methods of to-day instead of these observations we would have exact measurements of the scratches: their depth and length would be carefully ascertained, and finally the Carnegie Institution would be asked to make a grant for weighing the stone. In this way note-books would be filled and a reputation made, but who will say all this is worth as much as what Agassiz saw with his unaided eye? Logic has pitfalls for all of us: we escape from our errors only by shrewd observations and multiple verifications.

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MIASTOR LARVÆ

THESE remarkably interesting larvæ, reproduced by pedogenesis, are available for laboratory work to a marked degree and must be widely distributed as well as allied forms. Very little is known concerning American species, largely because their habitat is one rarely explored by entomologists. They breed mostly in decaying vegetable matter. We have been very successful in finding them under partially decayed chestnut bark of stumps, fence rails and sleepers which have been cut one or two years earlier. European species have been observed under the bark of a variety of trees and even in sugar beet residue. These dipterous maggots with diverging antennæ have a flattened, triangular head quite different from the strongly convex, usually fuscous head of the *Sciara* larvæ occurring in a similar environment. They have a length of from one twentieth to one eighth of an inch and may be found in colonies containing a few large, white larvæ with numerous smaller, yellowish individuals, though the latter appear more common at the present time. Early spring with its abundance of moist bark appears to be the most favorable season for finding the larvæ. The writer would welcome the cooperation of entomologists and others in searching for these forms in different parts of the country. He will be pleased to determine specimens found under various conditions, make rearings therefrom if possible, and thus add to our knowledge of the subfamily Heteropezinæ, a group which should be fairly abundant in North America and one deserving careful study.

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SCIENTIFIC BOOKS

Minéralogie de la France et des ses colonies; description physique et chimiques des minéraux; étude des conditions géologiques de leurs gisements. Par A. LACROIX. Paris, Librairie Polytechnique, Baudry et Cie, éditeurs. 1893–1910. Four volumes. 8vo. Pp. xx + 723; 804; vi + 815; iii + 923.

This monumental work, which testifies at once to the untiring industry of the writer and to his thorough mastery of the material he has collected, is destined to rank as one of the most valuable contributions to the science of descriptive mineralogy. It consists of four large volumes, containing in all about 3,300 pages, and illustrated with more than a thousand figures, a large number of which are photographic reproductions of characteristic specimens. The first volume was issued in 1893, and at that time the author believed that the work would be completed in two years' time by the issue of a second volume;